so-called rest state) to greater. It is not a measurable physical change, but a vital one which cannot be measured, and which *therefore* lies outside the scope of scientific knowledge. The two acts which seem to constitute the excitatory process, viz. excitation and response, are not continuous, but are joined together by a non-measurable link. This link is a subject of scientific conjecture, not of scientific knowledge; for nothing that is not measurable is known. It is, in short, something which is involved in *organism*, for which the most proper designation is ***organismal.***

"The point to be emphasised is that the organismal link or nexus is the *essential* part of the excitatory process; for neither the physical effect of the stimulus nor that of the response is effectual by itself. It is only when these two are coupled by the organismal nexus that the excitatory process is constituted.

"The propagation of the excitatory process thus constituted takes place, not through or by any measurable process, but is wholly and solely organismal, and therefore not measurable. The electrical machinulæ are acted on by the organismal stuff, and not by their neighbours. Propagation is a vital process, not a physical one.

"The purpose of the paper will be (in case it is ever written) to show (1) that the mere statement of measurable data stops short of its purpose because it misses the essential fact in the excitatory process; (2) that every electrical change accompanying excitation which is cyclical corresponds to a single organismal change; (3) that the organismal change is modified by (*a*) exhaustion and (*b*) injury, these being localised (*a*) at the proximal contact and (*b*) at the distal, and having opposite signs." (Here, of course, the ligatured muscle preparation is referred to.)

While your reviewer is certainly right in emphasising, as, indeed, we have done in the Memoir, Burdon Sanderson's strong objections to vitalism, it seems to me that in this letter he also lays his finger, deliberately and accurately, on the weak spot in the physico-chemical theory of life. It is the connection between "physical" or "chemical" stimulus and "physiological" response that is unintelligible from the point of view of the physico-chemical theory of life. Burdon Sanderson concluded that this connection lies outside the scope of scientific knowledge, and in this way he avoided the many scientific difficulties and defects of traditional vitalism. But we felt bound to point out the gap which is left if no attempt is made to deal scientifically with what he calls (the italics are his own) "the *essential* part of the excitatory process." In every department of physiology there is the same gap; and what remains for exact physical and chemical investigation would seem to be only the outer fringe of the real phenomena.

J. S. HALDANE.

Oxford, April 16.

A Peculiarity in the Shadows Observed during a Partial Eclipse of the Sun.

DURING the recent partial eclipse of the sun, I observed a peculiarity in the shadows cast by the sun's rays which may be worth recording. If eclipses occurred more frequently, this peculiarity would be familiar to everyone; as it is, I am not aware that it has been noticed, which is probably due to the fact that the attention of most observers was concentrated on the appearance of the sun itself.

While the eclipse was progressing I was walking along a country road white with dust. Along the edge of the road were young trees about 15 ft. high, decked with small, undeveloped leaves. The shadows of these trees, cast on the road, presented a peculiar appearance. What first attracted my attention was the number of salient angles in the shadows: these angles were not due to the shapes of the leaves, which were practically oval. A closer scrutiny revealed the fact that, where a leaf was isolated, its shadow took the form of a crescent; in fact, each such shadow was *a negative image of the visible portion of the sun's disc.*

This observation recalled to my mind a phenomenon to which my attention was directed some years ago by Mr. L. H. Winn. Mr. Winn observed that if a white screen be placed at some distance from a window which looks towards the sky, and a pencil be placed between the window and the screen, the shadow of the pencil takes the form of a faint negative image of the window, the vertical sashes being represented by bright lines, while the clear panes are represented by dark rectangles. Mr. Winn explained this phenomenon correctly by tracing the paths of individual rays; he also performed a number of other experiments which confirmed his reasoning. The explanation which will be given in this communication is of a somewhat more general character, and is a particular instance of Babinet's principle. The following laboratory experiments illustrate the phenomenon to be explained in a striking manner, and, at the same time, suggest its explanation.

Let a magic lantern (preferably illuminated with an arc lamp) be directed towards a white screen at a distance of 15 to 20 ft. Remove the focussing lens and cover the condenser with tissue paper. Next, let the circular illuminated area of the tissue paper be partly covered with an opaque disc, so that a brightly illuminated crescent remains visible. Place a sheet of cardboard, in which a hole about a centimetre in diameter has been bored, between the lantern and the screen. An inverted positive image of the illuminated crescent is formed on the screen: this image is produced in accordance with the principles which are exemplified by the pinhole camera. When the perforated sheet of cardboard is removed, the illumination of the screen is approximately uniform; and if a small ball about a centimetre in diameter, suspended by a fine thread, is placed in the position previously occupied by the perforation in the screen, it will cut off those rays which, by themselves, would form a positive inverted image on the screen. The part of the screen which was previously rendered bright by the rays which passed through the perforation in the card is now rendered dark by the interception of these rays, and the shadow of the ball takes the form of a dark crescent, which is a negative inverted image of the bright crescent from which the rays emanate. The ball must be placed so far from the screen that the umbra of the shadow is not formed; for the rest, there is considerable latitude as to the position in which the ball is placed. A body of about the same size, but with a shape differing considerably from the ball, casts an identical shadow. If a larger body is used, its shadow still has the form of an inverted negative image of the extended source of light, but the definition is less perfect.

If the disc which partly covers the illuminated tissue paper be removed, and a sheet of card, in which a stencil letter has been cut, is substituted for it, an inverted negative image of the stencil letter is obtained on the screen. Instead of the stencil letter, we may use a letter painted in transparent red on the tissue paper, the remainder of the illuminated area being painted blue; in this case the shadow of the ball thrown on the screen takes the form of an inverted image of the letter in blue, on a reddish ground.

These experiments show that *when light from an*

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extended source throws the shadow of a small object on a screen, under such conditions that the umbra of the shadow is not formed, then the shadow is the negative inverted image of the source of light.

Another shadow phenomenon observed during the partial eclipse may be mentioned here, although its explanation is obvious. In cases where the leaves of trees were so far advanced that most of the sun's rays were intercepted by them, the rays which passed through the small apertures between the leaves formed on the ground positive inverted images of the visible part of the sun's disc. The oval patches of light seen on the ground beneath thick trees under ordinary conditions of sunlight are due to the same cause. During the eclipse, the rays of the sun reflected from the free surface of water in a small glass formed a positive inverted image of the visible crescent of the sun on the walls or ceiling of the room in which the glass was placed.

EDWIN EDSER.

Halo during the Solar Eclipse of April 17.

As NATURE contains no mention of the circular halo that appeared for about half an hour during the solar eclipse on April 17, the following facts may be of interest, for possibly the appearance was very local.

I was in the south of the Isle of Wight, at sea-level, and noticed, just as the air began perceptibly to cool, that a faint and very gauzy film of cloud collected round the sun. This was transparent enough for the sun to appear through it unmasked, but just dense enough somewhat to relieve the glare and make it possible to take hasty glances at the sun itself with the naked eye. After the clouds drifted into position, they remained through the whole time of the eclipse, but disappeared when the sun's warmth returned. Shortly after their accumulation I saw a perfectly circular halo; this was coloured, but the bands of colour were only red, yellow, and greenish. The halo gradually increased in apparent diameter until it faded, as the eclipse waned.

MARIE C. STOPES.

University College, London, April 27.

The Smoke Problem.

UNDER this head an anonymous notice appeared in NATURE of April 11 of a little volume by Mr. Ruston and myself, in which the reviewer refers to certain "weaknesses in what is otherwise so excellent a work." As the "weaknesses" form the bulk of the review, I have permission to try to explain them.

The first "weakness" is in reference to the origin of soot, which we ascribe partly to mechanical removal of dust and partly to incomplete combustion. The reviewer denies that coal-dust is a product of incomplete combustion, and also that tar and free carbon are formed in the destructive distillation of coal. I had imagined that tar and coke were among the principal by-products of the gas industry.

The next criticism occurs in the paragraph that follows, and refers to the amount of tar (we call "tar" the oil extracted by ether from soot and coal) in the original coals. The reviewer concludes, after citing some of our analyses, "surely the authors do not believe that a ton of these coals contains about a couple of gallons of ready-made tar." But suppose the authors have the weakness to accept the results of their analyses, what then? The reviewer offers no suggestion.

In the next paragraph the reviewer finds fault with the statement: "The chimney gases were drawn off at the rate of about a litre a minute (*i.e.* through a narrow brass tube), which would approach the speed of the gases passing up the flue." Although he is good enough to interpret the passage for us in the only way in which it could possibly be interpreted, he concludes with the remarkable *non sequitur* that "if the flue draught was a litre a minute, it is no wonder their figures are abnormal." Of course, there is no such suggestion that the flue draught was a litre a minute (which has no meaning; unless the area of the flue is known), nor is it so stated. Having made this gratuitous assumption, on what grounds does he conclude that our figures are abnormal? What are the normal figures? I believe that the figures of the late Sir W. Roberts-Austen and our own are the only records of the kind, and they substantially agree.

In the next paragraph, among other remarkable items of information, is the statement that "the percentage of soot to carbon burnt is of no practical importance. It is the percentage loss on the fuel used that is the important factor." The reviewer apparently fails to see that (1) the entire object of the experiments was to ascertain the amount of soot emitted, and (2) the percentage of soot on carbon burnt can be easily calculated on the fuel used if the amount of carbon in the fuel is known (as it was in every case).

I do not wish to extend this reply by referring to our other "weaknesses," which are of the same gross order. I can only thank the editor for his courtesy in giving me his permission to show how and where some of them, at least, may have had their origin.

J. B. COHEN.

IN the above remarks by Prof. J. B. Cohen on the review of "Smoke: a Study of Town Air," which appeared in NATURE of April 11, the reviewer is first taken to task for denying that "coal-dust is a product of incomplete combustion, and also that tar and free carbon are formed in the destructive distillation of coal." The passage in the review was: "Dust is not, as a rule, a product of incomplete combustion, nor is the tar and free carbon formed in the destructive distillation of coal." The reviewer is still of opinion that coal-dust is not a product of incomplete combustion; by a strong chimney draught some coal-dust may be drawn up the flue, but it has certainly not been produced by combustion (unless Prof. Cohen looks upon the natural formation of coal as a process of incomplete combustion). Prof. Cohen elects to read the second part of the sentence as a denial that tar and coke are formed during gas manufacture, but it is doubtful if anyone else will do so; the reviewer's statement is that the tar and free carbon formed in the destructive distillation of coal are not products of incomplete combustion.

The authors give analyses of the original coal used in some of their experiments, and amongst the constituents of the coal figure certain percentages of tar, in one case amounting to 1·64 per cent.; and the reviewer says: "Surely the authors do not believe that a ton of these coals contains about a couple of gallons of ready-made tar." To this Prof. Cohen replies: "But suppose the authors have the weakness to accept the results of their analyses, what then? The reviewer offers no suggestion." If the authors do believe it, I am afraid they would take any suggestion the reviewer could make as an impertinence.

In replying to the criticism with regard to the rate of flow of the chimney gases in the flue, Prof. Cohen quotes from the book, and inserts five words which make the meaning clear, but which were not in the original paragraph.

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